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On the Theory of Scales of Measurement

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FOR SEVEN YEARS A COMMITTEE of the British Association for the Advancement of Science debated the problem of measurement. Appointed in 1932 to represent Section A (Mathematical and Physical Sciences) and Section J (Psychology), the committee was instructed to consider and report upon the possibility of "quantitative estimates of sensory events"—meaning simply: Is it possible to measure human sensation? Deliberation led only to disagreement, mainly about what is meant by the term measurement. An interim report in 1938 found one member complaining that his colleagues "came out by that same door as they went in," and in order to have another try at agreement, the committee begged to be continued for another year.

For its final report (1940) the committee chose a common bone for its contentions, directing its arguments at a concrete example of a sensory scale. This was the Sone scale of loudness (S. S. Stevens and H. Davis. *Hearing.* New York: Wiley, 1938), which purports to measure the subjective magnitude of an auditory sensation against a scale having the formal properties of other basic scales, such as those used to measure length and weight. Again the 19 members of the committee came out by the routes they entered, and their views ranged widely between two extremes. One member submitted "that any law purporting to express a quantitative relation between sensation intensity and stimulus intensity is not merely false but is in fact meaningless unless and until a meaning can be given to the concept of addition as applied to sensation" (Final Report, p. 245).

It is plain from this and from other statements by the committee that the real issue is the meaning of measurement. This, to be sure, is a semantic issue, but one susceptible of orderly discussion. Perhaps agreement can better be achieved if we recognize that measurement exists in a variety of forms and that scales of measurement fall into certain definite classes. These classes are determined both by the empirical operations invoked in the process of "measuring" and by the formal (mathematical) properties of the scales. Furthermore—and this is of great concern to several of the sciences—the statistical manipulations that can legitimately be applied to empirical data depend upon the type of scale against which the data are ordered.

A Classification of Scales of Measurement

Paraphrasing N. R. Campbell (Final Report, p. 340), we may say that measurement, in the broadest sense, is defined as the assignment of numerals to objects or events according to rules. The fact that numerals can be assigned under different rules leads to different kinds of scales and different kinds of measurement. The problem then becomes that of making explicit (a) the various rules for the assignment of numerals, (b) the mathematical properties (or group structure) of the resulting scales, and (c) the statistical operations applicable to measurements made with each type of scale.

Scales are possible in the first place only because there is a certain isomorphism between what we can do with the aspects of objects and the properties of the numeral series. In dealing with the aspects of objects we invoke empirical operations for determining equality (classifying), for rank-ordering, and for determining when differences and when ratios between the aspects of objects are equal. The conventional series of numerals yields to analogous operations: We can identify the members of a numeral series and classify them. We know their order as given by convention. We can determine equal differences, as $8-6=4-2$, and equal ratios, as $8/4=6/3$. The isomorphism between these properties of the numeral series and certain empirical operations which we perform with objects permits the use of the series as a *model* to represent aspects of the empirical world.

The type of scale achieved depends upon the character of the basic empirical operations performed. These operations are limited ordinarily by the nature of the thing being scaled and by our choice of procedures, but, once selected, the operations determine

that there will eventuate one or another of the scales listed in Table 1.[1]

The decision to discard the scale names commonly encountered in writings on measurement is based on the ambiguity of such terms as "intensive" and "extensive." Both ordinal and interval scales have at times been called intensive, and both interval and ratio scales have sometimes been labeled extensive.

TABLE 1

Scale	Basic Empirical Operations	Mathematical Group Structure	Permissible Statistics (invariantive)
NOMINAL	Determination of equality	*Permutation group* $x' = f(x)$ $f(x)$ means any one-to-one substitution	Number of cases Mode Contingency correlation
ORDINAL	Determination of greater or less	*Isotonic group* $x' = f(x)$ $f(x)$ means any monotonic increasing function	Median Percentiles
INTERVAL	Determination of equality of intervals or differences	*General linear group* $x' = ax + b$	Mean Standard deviation Rank-order correlation Product-moment correlation
RATIO	Determination of equality of ratios	*Similarity group* $x' = ax$	Coefficient of variation

It will be noted that the column listing the basic operations needed to create each type of scale is cumulative: to an operation listed opposite a particular scale must be added all those operations preceding it. Thus, an interval scale can be erected only provided we have an operation for determining equality of intervals, for determining greater or less, and for determining equality (not greater and not less). To these operations must be added a method for ascertaining equality of ratios if a ratio scale is to be achieved.

In the column which records the group structure of each scale are listed the mathematical transformations which leave the scale-form invariant. Thus, any numeral, x, on a scale can be replaced by another numeral, x', where x' is the function of x listed in this column. Each mathematical group in the column is contained in the group immediately above it.

The last column presents examples of the type of statistical operations appropriate to each scale. This column is cumulative in that *all* statistics listed are admissible for data scaled against a ratio scale. The criterion for the appropriateness of a statistic is *invariance* under the transformations in Column 3. Thus, the case that stands at the median (mid-point) of a distribution maintains its position under all transformations which preserve order (isotonic group), but an item located at the mean remains at the mean only under transformations as restricted as those of the linear group. The ratio expressed by the coefficient of variation remains invariant only under the similarity transformation (multiplication by a constant). (The rank-order correlation coefficient is usually deemed appropriate to an ordinal scale, but actually this statistic assumes equal intervals between successive ranks and therefore calls for an interval scale.)

Let us now consider each scale in turn.

NOMINAL SCALE

The *nominal scale* represents the most unrestricted assignment of numerals. The numerals are used only as labels or type numbers, and words or letters would serve as well. Two types of nominal assignments are sometimes distinguished, as illustrated (a) by the 'numbering' of football players for the identification of the individuals, and (b) by the 'numbering' of types or classes, where each member of a class is assigned the same numeral. Actually, the first is a special case of the second, for when we label our football players we are dealing with unit classes of one member each. Since the purpose is just as well served when any two designating numerals are interchanged, this scale form remains invariant under the general substitution or permutation group (sometimes called the symmetric group of transformations). The only statistic relevant to nominal scales of Type A is the number of cases, e.g. the number of players assigned numerals. But once classes containing several individuals have

[1] A classification essentially equivalent to that contained in this table was presented before the International Congress for the Unity of Science, September 1941. The writer is indebted to the late Prof. G. D. Birkhoff for a stimulating discussion which led to the completion of the table in essentially its present form.

been formed (Type B), we can determine the most numerous class (the mode), and under certain conditions we can test, by the contingency methods, hypotheses regarding the distribution of cases among the classes.

The nominal scale is a primitive form, and quite naturally there are many who will urge that it is absurd to attribute to this process of assigning numerals the dignity implied by the term measurement. Certainly there can be no quarrel with this objection, for the naming of things is an arbitrary business. However we christen it, the use of numerals as names for classes is an example of the "assignment of numerals according to rule." The rule is: Do not assign the same numeral to different classes or different numerals to the same class. Beyond that, anything goes with the nominal scale.

Ordinal Scale

The *ordinal scale* arises from the operation of rank-ordering. Since any 'order-preserving' transformation will leave the scale form invariant, this scale has the structure of what may be called the isotonic or order-preserving group. A classic example of an ordinal scale is the scale of hardness of minerals. Other instances are found among scales of intelligence, personality traits, grade or quality of leather, etc.

As a matter of fact, most of the scales used widely and effectively by psychologists are ordinal scales. In the strictest propriety the ordinary statistics involving means and standard deviations ought not to be used with these scales, for these statistics imply a knowledge of something more than the relative rank-order of data. On the other hand, for this 'illegal' statisticizing there can be invoked a kind of pragmatic sanction: In numerous instances it leads to fruitful results. While the outlawing of this procedure would probably serve no good purpose, it is proper to point out that means and standard deviations computed on an ordinal scale are in error to the extent that the successive intervals on the scale are unequal in size. When only the rank-order of data is known, we should proceed cautiously with our statistics, and especially with the conclusions we draw from them.

Even in applying those statistics that are normally appropriate for ordinal scales, we sometimes find rigor compromised. Thus, although it is indicated in Table 1 that percentile measures may be applied to rank-ordered data, it should be pointed out that the customary procedure of assigning a value to a percentile by interpolating linearly within a class interval is, in all strictness, wholly out of bounds. Likewise, it is not strictly proper to determine the mid-point of a class interval by linear interpolation, because the linearity of an ordinal scale is precisely the property which is open to question.

Interval Scale

With the *interval scale* we come to a form that is "quantitative" in the ordinary sense of the word. Almost all the usual statistical measures are applicable here, unless they are the kinds that imply a knowledge of a 'true' zero point. The zero point on an interval scale is a matter of convention or convenience, as is shown by the fact that the scale form remains invariant when a constant is added.

This point is illustrated by our two scales of temperature, Centigrade and Fahrenheit. Equal intervals of temperature are scaled off by noting equal volumes of expansion; an arbitrary zero is agreed upon for each scale; and a numerical value on one of the scales is transformed into a value on the other by means of an equation of the form $x' = ax + b$. Our scales of time offer a similar example. Dates on one calendar are transformed to those on another by way of this same equation. On these scales, of course, it is meaningless to say that one value is twice or some other proportion greater than another.

Periods of time, however, can be measured on ratio scales and one period may be correctly defined as double another. The same is probably true of temperature measured on the so-called Absolute Scale.

Most psychological measurement aspires to create interval scales, and it sometimes succeeds. The problem usually is to devise operations for equalizing the units of the scales—a problem not always easy of solution but one for which there are several possible modes of attack. Only occasionally is there concern for the location of a 'true' zero point, because the human attributes measured by psychologists usually exist in a positive degree that is large compared with the range of its variation. In this respect these attributes are analogous to temperature as it is encountered in everyday life. Intelligence, for example, is usefully assessed on ordinal scales which try to approximate interval scales, and it is not necessary to define what zero intelligence would mean.

Ratio Scale

Ratio scales are those most commonly encountered in physics and are possible only when there exist operations for determining all four relations: equality, rank-order, equality of intervals, and equality of ratios. Once such a scale is erected, its numerical values can be transformed (as from inches to feet) only by multiplying each value by a constant. An absolute zero is always implied, even though the zero value on some scales (e.g. Absolute Temperature) may

never be produced. All types of statistical measures are applicable to ratio scales, and only with these scales may we properly indulge in logarithmic transformations such as are involved in the use of decibels.

Foremost among the ratio scales is the scale of number itself—cardinal number—the scale we use when we count such things as eggs, pennies, and apples. This scale of the numerosity of aggregates is so basic and so common that it is ordinarily not even mentioned in discussions of measurement.

It is conventional in physics to distinguish between two types of ratio scales: *fundamental* and *derived*. Fundamental scales are represented by length, weight, and electrical resistance, whereas derived scales are represented by density, force, and elasticity.

These latter are *derived* magnitudes in the sense that they are mathematical functions of certain fundamental magnitudes. They are actually more numerous in physics than are the fundamental magnitudes, which are commonly held to be basic because they satisfy the criterion of *additivity*. Weights, lengths, and resistances can be added in the physical sense, but this important empirical fact is generally accorded more prominence in the theory of measurement than it deserves. The so-called fundamental scales are important instances of ratio scales, but they are only instances. As a matter of fact, it can be demonstrated that the fundamental scales could be set up even if the physical operation of addition were ruled out as impossible of performance. Given three balances, for example, each having the proper construction, a set of standard weights could be manufactured without it ever being necessary to place two weights in the same scale pan at the same time. The procedure is too long to describe in these pages, but its feasibility is mentioned here simply to suggest that physical addition, even though it is sometimes possible, is not necessarily the basis of all measurement. Too much measuring goes on where resort can never be had to the process of laying things end-to-end or of piling them up in a heap.

Ratio scales of psychological magnitudes are rare but not entirely unknown. The Sone scale discussed by the British committee is an example founded on a deliberate attempt to have human observers judge the loudness ratios of pairs of tones. The judgment of equal intervals had long been established as a legitimate method, and with the work on sensory ratios, started independently in several laboratories, the final step was taken to assign numerals to sensations of loudness in such a way that relations among the sensations are reflected by the ordinary arithmetical relations in the numeral series. As in all measurement, there are limits imposed by error and variability, but within these limits the Sone scale ought properly to be classed as a ratio scale.

To the British committee, then, we may venture to suggest by way of conclusion that the most liberal and useful definition of measurement is, as one of its members advised, "the assignment of numerals to things so as to represent facts and conventions about them." The problem as to what is and is not measurement then reduces to the simple question: What are the rules, if any, under which numerals are assigned? If we can point to a consistent set of rules, we are obviously concerned with measurement of some sort, and we can then proceed to the more interesting question as to the kind of measurement it is. In most cases a formulation of the rules of assignment discloses directly the kind of measurement and hence the kind of scale involved. If there remains any ambiguity, we may seek the final and definitive answer in the mathematical group-structure of the scale form: In what ways can we transform its values and still have it serve all the functions previously fulfilled? We know that the values of all scales can be multiplied by a constant, which changes the size of the unit. If, in addition, a constant can be added (or a new zero point chosen), it is proof positive that we are not concerned with a ratio scale. Then, if the purpose of the scale is still served when its values are squared or cubed, it is not even an interval scale. And finally, if any two values may be interchanged at will, the ordinal scale is ruled out and the nominal scale is the sole remaining possibility.

This proposed solution to the semantic problem is not meant to imply that all scales belonging to the same mathematical group are equally precise or accurate or useful or "fundamental." Measurement is never better than the empirical operations by which it is carried out, and operations range from bad to good. Any particular scale, sensory or physical, may be objected to on the grounds of bias, low precision, restricted generality, and other factors, but the objector should remember that these are relative and practical matters and that no scale used by mortals is perfectly free of their taint.

Technical Papers

Apparent Visible Violet Radiation in the Recent Large Sunspot Group

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The appearance of well-defined colors in sunspots, while not unknown, is rare enough to be noteworthy. As far back as 1826 Capocci reported violet tints associated with umbrae; Secchi, at Rome, using a Merz polarizer, saw a rosy tinge to umbrae; while the celebrated Herr Schwabe of Dessau saw occasional umbrae which were reddish-brown. Miss Brown, of Harvard, noted occasionally red-tinted umbrae. Young saw *all* umbrae as deep purple rather than black (*1*), but was disposed to regard this color as false and caused by the secondary spectrum of achromatics. However, it is clear that the *occasional* nature of the phenomenon rather disposes of the argument in favor of color by contrast, overcorrected objectives, etc. Moreover, such colors often have been seen in spots immediately adjacent to others which showed no color at all.

The normal color scheme of a sunspot is black and gray, the umbra being black and the penumbra, gray to yellowish-gray. Although Young fancied all umbrae to be very dark purple, to most eyes they appear as simply black by contrast with the photosphere. Penumbrae normally vary from very light to dark gray, although the apparent colors of both umbrae and penumbrae will be modified by the color of the helioscope screen.

A recent example of color was afforded by the Great Group of spots which appeared in the sun's northern hemisphere from 29 January to 12 February, and which has now returned to view. These spots, visible to the naked eye, and among the largest ever recorded, caused considerable radio disturbance and induced a brilliant aurora which was plainly seen at the latitude of Baltimore.

The writer first saw these spots on 29 January, when they were just coming around the following limb. By 1 February they were far enough on the disc to be seen well. At that time it was discovered that the so-called Great Group really consisted of three distinct groups: two relatively small groups, followed by an enormous penumbral field which enveloped a number of umbrae. The two principal umbrae of this group were unmistakably of a very dark violet hue, although no other umbrae within the same penumbral field, nor the umbrae of the two groups preceding, showed any color whatever. The penumbra of the largest group was normally gray. The penumbral filaments were distorted and confused, and a brilliant flocculus-like mass was observed, superimposed above the penumbra in its approximate center.

On 2 February the entire penumbra of this group had taken on a red-violet tint, the principal umbrae remaining very dark violet. On the same date the penumbra was found to be much rifted and ragged, and shot through in spots by brilliant eruptions manifested as bright spots within the penumbra.

On 3 February the penumbra had become greatly enlarged in area and had also changed color, now appearing as brown-violet. The two principal umbrae remained dark violet, all other umbrae within the penumbral field appearing normally black. Brilliant spots, rifts, and lanes in the penumbra were frequent, and a brilliant, detached flocculus was seen suspended above the largest umbra. A similar flocculus was observed over the leading umbra of the group immediately preceding the penumbral field group, but no color whatever was seen in the former.

On the following day *all* of the umbrae within the penumbral field group had assumed dark-violet tones, the umbrae in the two smaller groups preceding remaining normally black. The penumbra of the largest group remained brown-violet and continued to exhibit confused filamentation and brilliant eruptions. A detached flocculus was observed over another umbra within the field. It was clear that very violent activity was in progress here, apparently in concert with emission of radiation in the violet and of the spectrum, with invisible but implied emission in the ultraviolet. On the night of 7 February, this group having just passed the meridian, a brilliant aurora was seen in Baltimore even above the city lights.

On 8 February, when this group was nearing the preceding limb, the color of the penumbra had faded out to a pale violet, the principal umbrae appearing dark violet. Again, no color was seen in the adjacent groups. On 4 March when this group was again in position for good observation, the penumbra was seen to be *varicolored*—a sort of orange-brown on the west, fading into neutral gray on the east. The three principal umbrae then visible were deep violet and appeared to be surrounded by a red haze. No color was seen in any other spots on the disc.

The writer would like to emphasize that at no time was color *anticipated*. Moreover, the only previous occasion on which well-marked color had been seen

was in 1928, when the writer was using the 8-inch Clark refractor of the Maryland Academy of Sciences.

The helioscope used in the observations of present date was one which has the effect of reddening the photosphere but is otherwise neutral. It consists of a thin glass slide, smoked to the proper density and mounted in an adapter which screws directly onto the eyepiece. The writer has found this type of screen very satisfactory, since it intercepts the heat perfectly and gives a beautifully defined, natural-colored image of the sun in which the photosphere appears as light orange. Umbrae normally appear black and penumbrae gray against the orange background. The telescope used is a 3-inch one giving splendid definition with very fine color correction. The objective was ground for the writer some years ago by the American Optical Company and stands up perfectly with respect to color on even such bright objects as Jupiter when seen against a dark sky. It is felt, therefore, that the observations cited above were observations of fact and not mere illusions.

Reference

1. YOUNG, C. A. *The sun.* New York: Appleton, 1881. Footnote, p. 117.

The Activity of Synthetic Folic Acid in Purified Rations for the Chick[1]

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Previous studies have shown that vitamin B_{10} and B_{11} preparations (*3*), impure folic acid (from University of Texas) (*4*), crystalline isolated third *L. casei* factor (*4, 6*), and pure isolated vitamin B_c (*5*) have activity for growth, feathering, and hemoglobin formation in growing chicks. These earlier studies were necessarily limited by the lack of pure material or sufficient quantities of the compound. Since synthetic folic acid,[2] which has been reported active for growth and hematopoiesis (*1*), has now become available, it is important to study the activity of this compound under our experimental conditions.

Experimental method. Day-old White Leghorn cockerels from a commercial hatchery were maintained in electrically heated cages (32–37°) with raised screen bottoms for three or four days on a basal ration with the following percentage composition: dextrin, 61; casein, 18; gelatin, 10; l-cystine, 0.3; salts V (*2*), 6; soybean oil, 5; α-tocopherol, 0.3 mg.; 2-methyl-1,4-naphthaquinone, 0.05 mg.; thiamin · HCl, 0.3 mg.; riboflavin, 0.6 mg.; Ca pantothenate, 2 mg.; choline, Cl, 150 mg.; nicotinic acid, 5 mg.; pyridoxine · HCl, 0.4 mg.; biotin, 0.02 mg.; and i-inositol, 100 mg. In addition 1,700 U.S.P. units of vitamin A and 170 A.O.A.C. units of vitamin D_3 were administered by dropper. Chicks within a 10-gram weight range were then divided uniformly into groups of six, and supplements were added to the diet as follows: synthetic folic acid at levels of 10–300γ/100 grams of ration, isolated vitamin B_c, and preparations of vitamins B_{10} and B_{11} containing different amounts of "folic acid"[3] which were fed for comparison.

The three experiments were terminated when the chicks were four weeks old. The methods used for determining hemoglobin and measuring feather development were the same as those used previously (*3*). The hematocrit determinations were made following the procedure of O'Dell, *et al.* (*9*).

Results. The addition of 25γ of synthetic folic acid to our basal ration prevents the reduced growth, poor feathering condition, and low hemoglobin and hematocrit values which are consistently obtained when this ration is fed to chicks. Twenty-five micrograms of isolated crystalline vitamin B_c gave a similar effect, although the growth was slightly less than in chicks fed similar levels of synthetic folic acid. When vitamin C or whole liver powder was fed with folic acid, the chicks grew somewhat better than when fed folic acid alone (compare Groups 10 and 11 with 4). The addition to the diet of 50γ of α-pyracin (2-methyl-3-hydroxy-4-hydroxymethyl-5-carboxypyridine) alone or with 10 or 50γ of synthetic folic acid produced slightly better growth but no improvement in feathering or hemoglobin. When 500γ of α-pyracin were fed with the basal ration alone, no response in growth, hemoglobin, or feathering was noted (Group 14).

Neither *p*-aminobenzoic acid nor a vitamin B_{10} and B_{11} concentrate gave a significant supplementary effect in the presence of an adequate amount of synthetic folic acid. Further comparison of the addition of vitamin B_{10} and B_{11} preparations without added folic acid (Groups 19 and 21) shows clearly the lack of

[1] Published with the approval of the director of the Wisconsin Agricultural Experiment Station. The study was supported in part by grants from the Wisconsin Alumni Research Foundation and Swift and Company.

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[2] Synthetic folic acid was originally called *L. casei* factor.

[3] "Folic acid" (in quotes) is used throughout the paper to designate the microbiological activity of a material when compared to synthetic folic acid on a weight basis (*7*).

correlation between chick growth and the microbiological activity. Chicks in Group 19, which received in the supplement less than 5γ of "folic acid" (as measured microbiologically after acid hydrolysis, *8*), per 100 grams of ration, grew as well, if not better, than the chicks in Group 20, which received a supplement containing 61γ of "folic acid" per 100 grams of diet.

TABLE 1

RESULTS OF FEEDING SYNTHETIC FOLIC ACID AND RELATED COMPOUNDS TO CHICKS

No.	Supplement per 100 grams ration	No. chicks	Av. wt. grams 4 wks.	% B_{10}*	Gram % Hb ± 1	Ht %†
1	None	18	150	40	6.6	20
2	10γ synthetic folic acid .	17	215	65	8.4	
3	15γ " " " .	6	205	85	8.3	
4	25γ " " " .	17	250	90	8.4	26
5	50γ " " " .	12	235	100	9.0	27
6	100γ " " " .	5	250	100	8.9	
7	300γ " " " .	6	260	100	8.1	26
8	10γ vitamin B_c	6	180	55	8.6	
9	25γ " "	6	225	90	8.8	
10	25γ synthetic folic acid + 100 mg. vitamin C	6	290	95		
11	25γ synthetic folic acid + 10 grams whole liver powder	6	275	100	8.5	28
12	25γ synthetic folic acid + 5 mg. P.A.B.A. ..	6	250	90		
13	50γ α-pyracin	6	185	45		
14	500γ α-pyracin	12	155	40	6.6	
15	50γ α-pyracin‡ + 10γ synthetic folic acid ...	6	235	45	8.7	
16	50γ α-pyracin + 50γ synthetic folic acid ...	6	260	100	8.7	
17	Vitamin B_{10} and B_{11} prep.§ ≅ 5% liver L (8γ "folic acid") .	18	260	90	9.1	
18	Vitamin B_{10} and B_{11} prep. ≅ 5% liver L (8γ "folic acid") + 100γ synthetic folic acid	6	270	100	8.7	30
19	Vitamin B_{11} prep.‖ ≅ ½% yeast extract (4.7γ "folic acid") .	6	240	30		
20	Vitamin B_{11} prep. ≅ ½% yeast extract + 10γ synthetic folic acid	6	200	65		
21	Vitamin B_{10} prep.¶ ≅ ½% yeast extract (61γ "folic acid") .	6	225	95		

* 0 = very poor; 25 = poor; 50 = fair; 75 = good; 100 = very good.

† See (8) for method used to determine hematocrit per cent.

‡ α-pyracin was obtained through the courtesy of Dr. Karl Folkers at Merck and Company, Inc.

§ Prepared following the direction for the superfiltrol eluate from liver fraction L (3).

‖ Vitamin B_{11} preparation No. 284–5 was made by autoclaving Difco Yeast Extract in 1 N/KOH for one hour at 15 lb.

¶ Vitamin B_{10} preparation No. 298 was made by treating Difco Yeast Extract with chicken pancreas and incubating at 37° for 70 hours at pH 7.

When sulfasuxidine (succinylsulfathiazole) was included in the basal diet, the chicks receiving no supplement developed a more severe deficiency, and under these conditions 25γ of synthetic folic acid per 100 grams of diet did not give a complete response. Fifty micrograms of synthetic folic acid were not as effective in the ration containing 1 per cent of sulfasuxidine as in the diet containing 0.5 per cent of the sulfa drug. A comparison of the activity of synthetic folic acid with and without the addition of 1 per cent sulfasuxidine in the diet (Fig. 1) indicates that folic acid is about three times as effective without the drug.

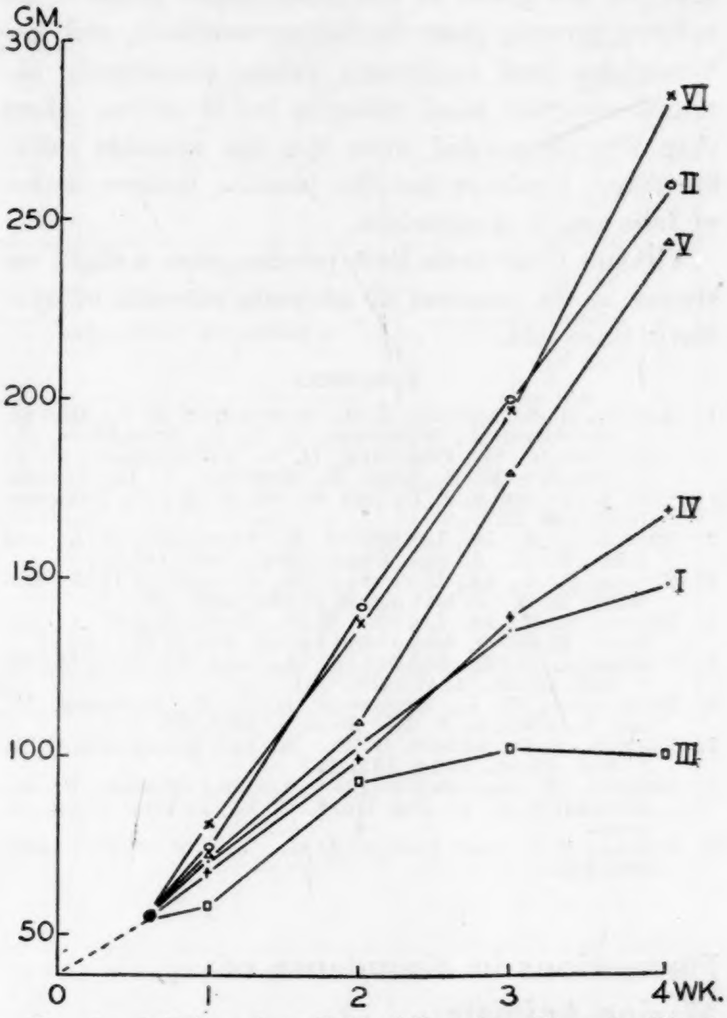

FIG. 1. Activity of synthetic folic acid with and without sulfasuxidine. I = basal ration; II = basal ration + 25γ per cent synthetic folic acid; III = basal ration with 1 per cent sulfasuxidine; IV = III + 25γ per cent synthetic folic acid; V = III + 50γ per cent synthetic folic acid; VI = III + 100γ per cent synthetic folic acid.

Discussion. The results presented here clearly demonstrate that chicks given our basal ration show significant responses in growth, feathering, and hemoglobin formation when 25γ of synthetic folic acid are given. It is still important to decide whether the compound functions directly to produce all these responses or whether the activity may be indirect to some extent. In fact, the increased requirement with the sulfasuxidine suggests some degree of indirect activity. The reason the third *L. casei* factor and the Texas folic acid preparation appeared to be inactive in our earlier studies (*4*) may now be explained on the basis of the increased requirement of folic acid in the presence of sulfasuxidine.

The existence of microbiologically inactive compounds which produce feathering (vitamin B_{10}) and growth (vitamin B_{11}), of highly active synthetic folic

acid, and of preparations with high bacterial potency but relatively little chick activity makes it necessary to determine which or how many of these compounds are used directly and the possible interrelationships between these compounds.

Summary. The addition of 25γ of synthetic folic acid per 100 grams of our basal ration prevents the reduced growth, poor feathering condition, and low hemoglobin and hematocrit values consistently obtained when the basal ration is fed to chicks. More than 25γ are needed when the diet contains sulfasuxidine. Evidence for the possible indirect action of folic acid is summarized.

Vitamin C or whole liver powder gives a slight response in the presence of adequate amounts of synthetic folic acid.

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Fluctuations in Abundance of Marine Animals

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Bingham Oceanographic Laboratory, Yale University, and The Chesapeake Bay Fisheries Commission

The starfish (*Asterias forbesi* Desor) is at times a source of serious injury to the mollusk fisheries of southern New England. The precise extent of the damage done by this pest is extremely difficult to assess, but the cost of efforts at control provides some indication of the magnitude of the problem. The oyster growers of Connecticut alone are conservatively estimated to spend an average of $100,000 per year in combating starfish on their private beds; and approximately $75,000, independent of Federal grants, was paid out for the removal of starfish from public bottoms by the authorities of Massachusetts and Rhode Island during the decade from 1932 to 1942.

It has long been recognized by oyster growers that the amount of trouble caused by starfish is not constant but rises from time to time to the proportions of a plague. Until the present, however, no attention has been given to the pattern of these fluctuations. Evidence here presented in part (Fig. 1) seems to demonstrate that troughs and peaks of abundance of starfish have alternated at seven-year intervals, since 1852 at least, in synchrony throughout the region from New York to Cape Cod. The regularity of these changes during the past hundred years suggests that potential damage from starfish may be expected to decline to a minimum around 1950 and to increase thereafter to a peak during the years around 1957. It is believed that ability to foretell in a general way the times of danger from starfish may effect considerable direct and indirect savings to the New England oyster industry, by permitting a more efficient planning of the use of vessels, oyster stock, and oyster grounds.

Adequate statistical information upon starfish is not available. Definition of periods of scarcity and of abundance of starfish upon the oyster beds has, nevertheless, been found possible by the systematic employment of semiquantitative data, the value of which for such purposes has not heretofore been generally appreciated. These nonnumerical data are derived from statements concerning relative amounts of trouble with starfish at various points in southern New England, contained in reports of public commissions, trade journals, newspapers, periodicals, private memoranda, etc. As examples of such statements, the *Norwalk Gazette* for 3 July 1858 reports that "the star-fish are eating up the oysters in the beds of New York harbor . . . [and in consequence the] proprietors have petitioned the state to remit the usual taxes . . ."; and the Reports of the Rhode Island Commissioners of Shellfish for 1859 and for 1861 state that "during the past year [1859] a very large quantity of the oysters planted upon the private oyster beds [of Narragansett Bay] have been destroyed by the star fish . . . [so that] the past two years have been . . . years of want . . . ," and that "for some time prior to . . . 1860 the increase of the star-fish had been very rapid, until, in that year, they became so numerous and so destructive as to render an entire abandonment of the ground necessary."

One hundred and eighty-five such statements have been collected, tabulated chronologically, compared, and evaluated in terms of "Many," "Few," etc. Although these evaluations are necessarily subjective, the indications upon which they are based are in general quite unequivocal; and although the reliability of individual indications might be questioned, there is in general a close agreement between independent statements from the same as well as from different parts of New England during the same period. A

summary of the evaluations is presented graphically in the lower half of Fig. 1. The upper half of the figure shows the quantities of starfish caught annually by an oyster company in Connecticut, 1892–97, and by one in Rhode Island, 1908–38.

Nonnumerical data on starfish are obviously in-

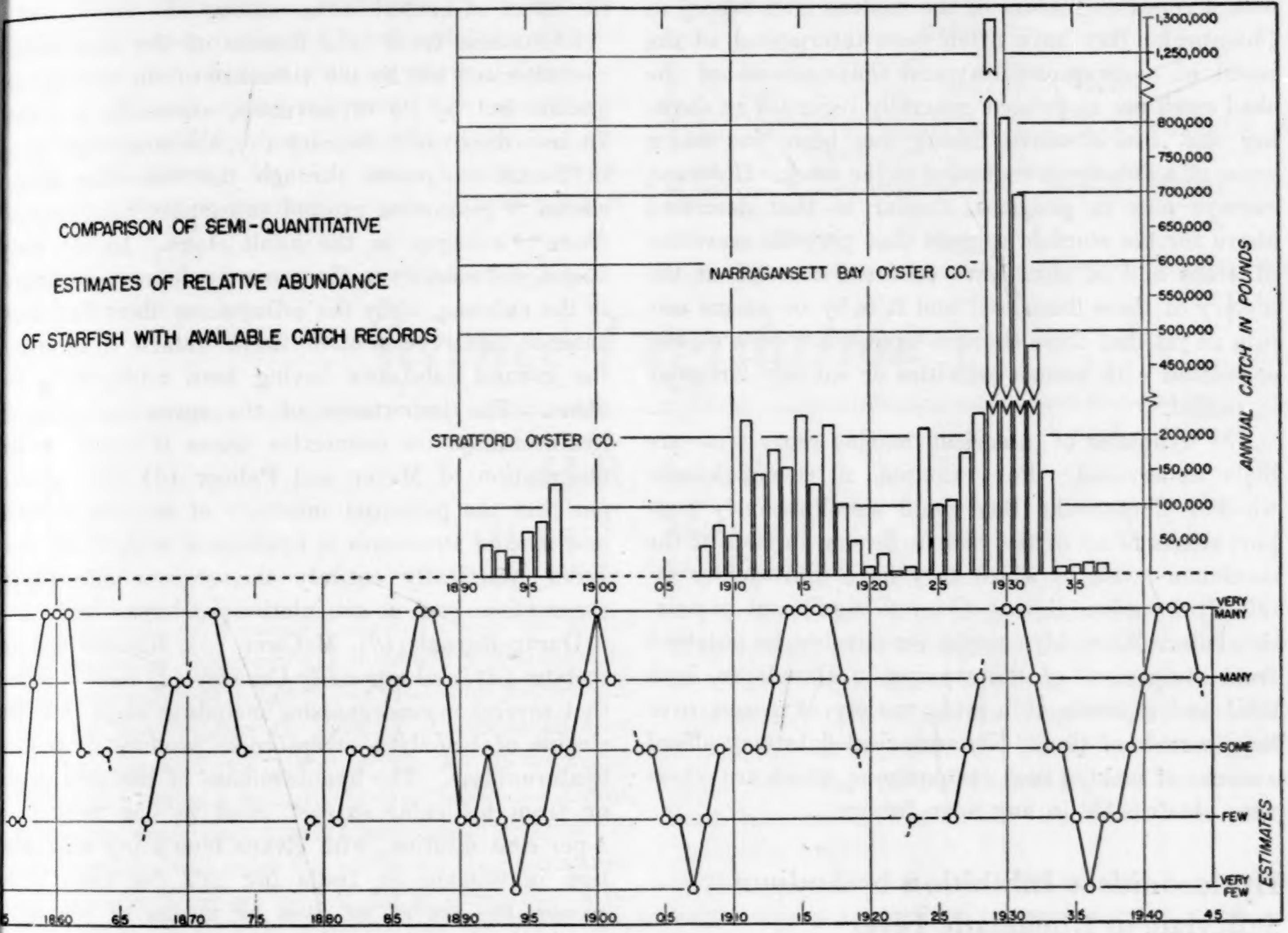

FIG. 1

capable of defining absolute differences between different periods or stations of observation, but they do deal directly with immediate trends in the degree of trouble experienced. The available numerical data are for limited times and localities only and are not corrected for variations in fishing effort, but they do show clearly that there were great and real changes in the quantity of starfish taken from certain oyster beds. The general agreement between the two types of record seems to justify the conclusion that cyclical changes in the population of starfish have actually occurred. It will be seen from Fig. 1 that these changes have been of such regularity that, by successively adding 14 years to a given date (for example, 1858, 1872, 1886, 1900, 1914, 1928, 1942), the entire record can be traversed without loss of phase. The long (14-year) period of the cycle, its high degree of regularity, and the fact that the cycle concerns a marine invertebrate, are all unusual features.

The data here described do not allow distinction to be made between changes in abundance and changes in distribution, but there is considerable evidence to support the hypothesis that there is a close relation between amount of trouble with starfish and magnitude of the general population of this animal. Calculations concerning the stock of starfish suggest that the biomass during average peak periods may be of an order 20 times as great as that during average troughs; and that the peak populations may reach or surpass the tremendous average density of 1,000 pounds per acre on the half million acres where the species is common in southern New England (a density of about the same order as that during the unique outbreak of the same species in Chesapeake Bay in 1936–38). Knowledge of this regular pattern of violent changes in the stock has opened the way to new conceptions of the natural history of the starfish and the problems of its control, to be reported in forthcoming publications.

Lack of long-term continuous statistics upon abun-

dance is not limited to such relatively obscure forms as the starfish. Indeed, available numerical information by no means excludes even the possibility that regular patterns of natural fluctuation might exist in various commercially important populations, such as those of crab, lobster, shad, croaker, etc. For example, the catastrophic declines in production which have been a repeated feature of the modern crab fishery in Chesapeake Bay have often been interpreted as the result of overexploitation; and the statistics of the shad catch are at present generally regarded as showing that overintensive fishing has been the major cause of a disastrous reduction in the stock. However, surveys now in progress, similar to that described above for the starfish, suggest that periodic scarcities of crabs and of shad have occurred throughout the history of these fisheries;[1] and it is by no means certain as yet that these changes have either been closely correlated with human activities or entirely irregular in period.

The dynamics of American marine ecosystems are little understood. For example, it is not known whether Chesapeake Bay could simultaneously support stocks of all of the various fishery animals at the maximum levels to which they have individually attained at various times. Clues to significant population interrelationships might conceivably be obtained from comparison of the changes in abundance, both local and general, of a wide variety of forms over long periods of time. Nonnumerical data may afford a means of making such comparisons, which are otherwise not feasible in any near future.

Hyaluronidase Inhibition by Sodium Salicylate in Rheumatic Fever

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The predominating role of the mesenchyme tissues in rheumatic fever and the permeability changes that are known to occur in its evolution point to interrelationship of several characteristics of rheumatism and the spreading factors of connective tissue, whether of bacterial or endogenous origin. On the other hand, no clear explanation has been presented for the mechanism of sodium salicylate or related drugs in rheumatic fever, although the arresting of the inflammatory process and the regression of symptoms are not the effect of bacteriostatic action.

Rheumatic fever is a disease of the mesenchyma characterized not by the virulence of the etiologic organism but by its invasiveness, especially in youth. In accordance with Bensley (*1*), the connective tissue in formation passes through the following stages: edema → gelatinous ground substance → argyrophilic fibers → collagen in the adult stage. In the early stages, diffusibility in the ground substance is favored to the extreme, while the collagenous fibers that characterize adult connective tissue retard diffusibility, the ground substance having been replaced by the fibers. The importance of the spreading effect of hyaluronidase on connective tissue is based on the observation of Meyer and Palmer (*6*) who pointed out that the principal substrate of connective tissue and mucoid structures is hyaluronic acid, which composes practically entirely the regions affected by rheumatism, such as articulations and synovial fluid.

Durán-Reynals (*3*), McClean (*5*), Kendall and associates (*4*), and especially Crowley (*2*) have observed that several microorganisms including more than 200 strains of hemolytic streptococci produce or possess hyaluronidase. The hyaluronidase of bacterial origin or from testicular extract, used in our work in a 1-per cent dilution, with Evans blue 1-per cent solution in humans or India ink 1:2 for rabbits, increases the spread of dyes by means of enzymatic activity hydrolizing the hyaluronic acid present in the ground substance, decreasing the viscosity, and thus favoring the passage of liquids, exudates, and pathogenic microorganisms. Its action may be divided into the following stages: (a) decreasing acetic acid coagulation of the substrate, (b) decreasing viscosity, and (c) hydrolizing of hyaluronic acid with the release of glucosamine and reducing substance.

In a total of 96 experiments on 24 albino rabbits it was observed that the spread area of India ink with hyaluronidase was six times greater than with saline. The oral or intravenous administration of sodium salicylate inhibited by 57 to 66 per cent the spreading effect of hyaluronidase; the degree of inhibition varied with the dose of salicylate administered. Sulfadiazine did not reduce the activity of hyaluronidase, but appeared to enhance its effect with inflammatory reactions in the center of the area in several groups.

[1] Thus, in the case of the Hudson shad run, what may have been a general scarcity during the 1830's has been noted by DeVoe (*The market assistant*, 1867, p. 200): "This shad-fishery [Cortelyou's fyke-nets in the Narrows] has been gradually decreasing since the year 1824, so that now [1838] it is scarcely worth attending to. . . . All the fisheries in New York harbor are nearly destroyed and the fish which now supply the markets of the city are brought from the distance of sixty, eighty, and even a hundred miles. . . ." Again, during the latter third of the century, "Great complaint and dissatisfaction were encountered everywhere [on the Hudson], the fisheries having fallen off immensely. . . . There never before had been so few shad taken and the retail price in market rarely fell below seventy-five cents for fish which ten years ago [1858] were sold for ten and fifteen cents apiece . . ." (*Report of the Commissioners of Fisheries of New York*, 1869, pp. 9–10). During the late 1870's and 1880's there followed what appears to have been a gradual renewal of abundance, which was credited at the time to hatchery operations stimulated by the preceding scarcity. A fresh decline and a fresh recovery have followed in this century.

These results were reproduced in a total of 144 experiments on 36 normal male and female adults and children employing the same criteria of the spreading effect of Evans blue with hyaluronidase reached by intravenous injections of 275 γ/cc. of sodium salicylate in plasma and then measuring the areas after 24 hours of the intradermal injection of the dye with the enzyme. Intradermal injections on individuals, either with active rheumatic fever or having suffered it gives unique reactions with enormous diffusion of the dye and local edema that sometimes occupies the arm injected with hyaluronidase. The salicylate also inhibits the enzyme in those cases and reduces its spreading effect on connective tissue. These types of allergic reactions to hyaluronidase were also observed in one male who suffered exanthematic typhus.

The evidence found in normal rabbits and humans, as well as in individuals who have latent or active rheumatic fever, indicates the important role of hyaluronidase in its mechanism and the inhibitory effect of sodium salicylate as a typical antirheumatic drug.

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Science Legislation

H.R. 6448

Howard A. Meyerhoff
Executive Secretary, AAAS, Washington, D. C.

The passage of a national science foundation bill in this session of Congress was seriously jeopardized by the introduction of a bill in the House of Representatives by Rep. Wilbur D. Mills of Arkansas 15 May. The bill was referred to a subcommittee of the Interstate and Foreign Commerce Committee of the House, which has since held hurried hearings. Whether the subcommittee will refer the bill to another committee, whether it will report the bill out of committee in the near future, or whether it will let the bill die in committee is not known at the present time.

However, the whole procedure in this, the first introduction of the House of Representatives to national science foundation legislation, is so reminiscent of that which surrounded the introduction of the May-Johnson bill that readers of *Science* need to be informed about it.

The facts surrounding the introduction of the bill and the subsequent hearings bear the following relations to legislation which is pending in the Senate.

In the first place, the bill was handed to Representative Mills by an ardent and unreconstructed proponent of the old—and abandoned—Magnuson bill, S. 1285. This same proponent generously volunteered to arrange for witnesses of his selection to appear at hearings which were held without the knowledge of the departments of government ordinarily consulted in such matters, for example, Interior, Commerce and Agriculture.

Needless to say, none of the sponsors of S. 1297 (the Kilgore bill) were among the few who testified: None of those groups who merely sought sound legislation without favoring either the Kilgore bill or the Magnuson bill was represented. On the other hand, influential opponents of the Kilgore bill and the more recent Kilgore-Magnuson bill (S. 1850) were among those who testified.

The proponent of H.R. 6448-S. 1285 is not a scientist, nor is he qualified to speak for the great body of American scientists. Somehow, through Rep. Mills he obtained the introduction of a bill for which he was prepared to secure expert testimony in advance. In this high-minded and democratic manner the House has obtained its initial introduction to the National Science Foundation.

It is perhaps fortunate that the week which was scheduled to be "Science Week" in the Senate (See *Science*, 1946, **103**, 589) was lost to more pressing labor problems, for although the delay may mean that a national science foundation will not be created in 1946, at least scientists will have time to acquaint their congressmen with their views. If there has been any doubt that this must be done in the minds of any individual scientist, the introduction of H.R. 6448 should dispel it. Science legislation is "on the loose."

Do scientists want what the National Association of Manufacturers, or some few persons whether they are scientists or not, believe that they *should* have? The 232 members of the AAAS Council who favored action

to assure passage of S. 1850 don't believe so. The many members of the Association who have written and spoken to the writer don't believe so.

If individual scientists are to get what they *do* want, they must become articulate. They must become more vocal with their congressmen than the willful individuals or the reactionary organizations who may yet lobby objectionable and obstructive legislation onto the statute books,—and who are evidently determined to do it!

The following people testified on 28 May in connection with the bill **before the Subcommittee on Public Health of the Interstate and Foreign Commerce Committee: Rep. Thomas A. Jenkins, of Ohio; Dr. Isaiah Bowman, president of Johns Hopkins University; Rep. Wilbur Mills, of Arkansas; Mr. Robert T. Patterson, Secretary of War; Dean C. D. MacQuigg, of Ohio State University; Dr. Homer Smith, of New York University College of Medicine; Dr. R. E. Dyer, of the National Institute of Health; Dr. Vannevar Bush, director of the Office of Scientific Research and Development; Mr. W. J. Kenney, Assistant Secretary of the Navy, and Mr. John Victory, executive secretary of the National Advisory Committee for Aeronautics.**

The hearings continued on 29 May, when the following testified: Mr. George E. Folk, of the National Association of Manufacturers; Dr. Detlev Bronk, director of the Johnson Research Foundation of the University of Pennsylvania, and Dr. A. N. Richards, vice-president of the University of Pennsylvania and chairman of the Committee on Medical Research of the Office of Scientific Research and Development. Dr. Bronk and Dr. Richards spoke in broad terms which would apply equally to H. R. 6448 or S. 1850.

Dr. E. U. Condon, director of the National Bureau of Standards, also testified on 29 May, but opposed the Mills bill, pointing out that it was practically identical with the Magnuson Bill, which contained features unsatisfactory to the majority of American scientists. Dr. Condon spoke both for himself and for Secretary Wallace, whose written statement he read. Secretary Wallace pointed out in his statement why the compromise Senate bill S. 1850 was a better bill than the proposed legislation in the House, based as it is on the older discarded Magnuson bill.

As the situation now stands, all of these people are subject to recall to testify before the committee again at its pleasure. It is hoped that *Science* can print excerpts from the testimony referred to above in the issue of 14 June.

Scientists Divided

Watson Davis
Director, Science Service, Washington, D. C.

Another science foundation bill to provide federal funds for scientific research is making a bid for Congressional action, although a compromise bill, S. 1850, worked out by months of Senate hearings to reconcile viewpoints among scientists has already been reported out of committee (19 March) and awaits a place on the Senate calendar (*Science*, 1946, **103**, 382; 589).

This new bill, H.R. 6448, introduced by Rep. Wilbur D. Mills of Arkansas 15 May, has had two days of hurriedly called hearings, at which Dr. Vannevar Bush, director of the OSRD gave it his blessing as "fulfilling the needs of the country better than any other piece of legislation I have seen for the purpose."

A statement from Secretary of Commerce Wallace, presented by Dr. E. U. Condon, Bureau of Standards director, declared the new bill "will not foster the progress of free scientific inquiry" and "will lead to an increasing monopolization of science by a small clique and operate to the detriment of small and independent business in this country."

The Mills bill puts the control of the proposed national science foundation in the hands of a part-time board, whereas the Senate bill (1850), compromising earlier Magnuson and Kilgore bills, provided a full-time Presidentially-appointed administrator with a part-time advisory board.

The new bill continues the present general practice under which commercial patent rights go to the inventor, meaning generally the institution where the research is done, with the government getting only the right to use the results for its own purposes. The Senate bill (1850) provides generally that federally-supported research shall be freely available to the public.

The new bill does not distribute funds geographically or to non-profit organizations, and it does not provide for support of social science research.

A National Association of Manufacturers representative, George E. Folk, testified for the new bill, and so did Prof. Homer W. Smith, New York University physiologist, who has acted as secretary of the Committee Supporting the Bush Report.

Last November this Committee, headed by President Isaiah Bowman of Johns Hopkins University, attacked the Kilgore bill and supported the Magnuson bill. After the compromise S. 1850 bill had been developed, this group as recently as 23 April on behalf of 5,000 scientists, appealed to Congress for its enactment.

The stand of Dr. Bush, Prof. Smith and others upon the Mills bill seems to again divide those who favor some sort of National Science Foundation.

News and Notes

The Senate unanimously passed the McMahon bill for the domestic control of atomic energy in a surprise move Saturday afternoon 1 June. The Selective Service bill which was to have been debated on Saturday was deferred until Monday, so that administration leaders had an opportunity to put the position of the United States on record before its policy is presented to the United Nations Atomic Energy Commission on 14 June. The McMahon bill now goes to the House of Representatives where it will be referred to the House Military Affairs Committee. (A detailed analysis of the provisions of the McMahon bill was published in *Science*, 1946, **103**, 133.) Two amendments were made on the Senate floor. One, sponsored by the McMahon Committee, prohibits any person from manufacturing or acquiring fissionable material or from using atomic energy in a weapon without a license from the Atomic Energy Commission. The other amendment, offered by Senator Joseph C. O'Mahoney of Wyoming, would prohibit any person or corporation associated with wartime atomic bomb projects from asserting claims to any land which might contain raw materials for the manufacture of fissionable products.

In his speech for the bill Senator McMahon emphasized the biological and medical application of radioactive isotopes, although he also mentioned peacetime uses of nuclear energy for power.

Senator Vandenberg said that he hoped that the Senate action represented a transitional phase of atomic control and that this control would soon pass into the hands of the United Nations Commission.

Alexander G. Langmuir, a member of the Commission on Acute Respiratory Diseases at Fort Bragg, North Carolina, under the Army Epidemiological Board, has been appointed an associate professor in the Department of Epidemiology of The Johns Hopkins School of Hygiene and Public Health.

John Chipman, professor of process metallurgy at the Massachusetts Institute of Technology since 1937, has been appointed head of the Institute's Department of Metallurgy. Dr. Chipman will succeed Robert S. Williams, who retires on 1 July after 44 years of service.

Reynold C. Fuson, University of Illinois, will receive the honorary degree of D.Sc. at Montana State University's commencement exercises on 10 June. Dr. Fuson was graduated from Montana State University in 1920 and has been on the faculty at the University of Illinois since 1927.

Announcements

Edith S. Clements has presented to the Department of the Natural Sciences of Santa Barbara College a large collection of literature and maps and a quantity of laboratory instruments selected from the library and laboratory of her late husband, Frederic E. Clements. The material constitutes the Frederic E. Clements Memorial, dedicated to the encouragement of the study of plant ecology.

Particularly useful is the large collection of pamphlets and reprints from scientific journals collected throughout the long career of the Drs. Clements. These comprise a high percentage of the widely scattered literature on plant ecology, the Scandinavian, Swiss, and Slavic writings being particularly well represented. Also quite valuable is the great mass of descriptive literature dealing with the vegetation of North America. Probably between 3,000 and 4,000 items, ranging from reprints to whole volumes, many of them not yet classified, are included in the library collection.

Dr. Edith Clements has expressed a strong desire that her gift be utilized to the fullest extent, and to this end it is being organized for class use in courses in plant ecology and as a reference collection for research. It will also be available for the use of visitors and for interlibrary loan except in the case of fragile items. The map collection and the instruments will serve as the nucleus of the teaching aids and materials to be employed in a series of courses in plant ecology.

Under a contract with the War Department, a research program concerned with the development of standard laboratory methods for evaluating rat-repellent substances is now getting under way at the University of Pittsburgh. The program is sponsored by the Office of the Quartermaster General. Wayne Dennis, head of the Department of Psychology, is chairman of the advisory committee for the project, other members of the committee being Robert A. Patton, lecturer in psychology, and Roger W. Russell, assistant professor of psychology. Lawrence M. Stolurow has been appointed research psychologist for the project.

Rohrer, Hibler, and Replogle, consulting psychologists, announce the following additions to the staff: Edward M. Glaser, Los Angeles office; Mack T. Henderson and Ellwood W. Senderling, Chicago office; Richard W. Wallen, Cleveland office; and J. Watson Wilson, New York office.

The American Meteorological Society, through its president, H. G. Houghton, of Massachusetts Institute of Technology, wishes to call to the attention of American colleges the fact that a large number of carefully selected college students were given training in professional meteorology at a few selected universities in this country, preparatory to their employment by the Army and Navy Meteorological Services. A number of these men, before entering the meteorological field, had done a considerable amount of graduate work in physics, chemistry, biology, engineering, geology, etc. and are now available for teaching and research in these fields.

In most of our colleges which offer nonprofessional courses in meteorology to meet the needs of students in geography, geology, and in agricultural subjects, it has been standard practice to have these nonprofessional courses taught by members of various departments. The return of many of the professionally trained service meteorologists offers an opportunity for our colleges to obtain the services of instructors who are capable of doing professional work in Departments of Physics, Chemistry, Geology, etc. and who at the same time are qualified, as experienced and trained professional meteorologists, to present stimulating nonprofessional courses in meteorology on a somewhat higher plane than may have been possible in the past.

The Society is anxious to cooperate with American colleges in providing information concerning these returning weather officers that might be helpful in placing them in suitable teaching positions. To that end, files have been kept which contain detailed information on the education and practical experience of these men.

Inquiries may be directed to the Executive Secretary, American Meteorological Society, 5 Joy Street, Boston 8, Massachusetts.

Word has been received from China regarding the last years of Prof. Amadeus W. Grabau, whose death occurred 20 March 1946 in Peiping (*Science,* 1946, **103,** 534). Prof. Grabau became a resident of China in 1920 and was, from that time on, chief paleontologist of the National Geological Survey of China and concurrently professor of paleontology in the National University of Peking. In 1937, when the Japanese took Peiping and North China by force, Prof. Grabau had been unable to leave that city because of illness and lack of communication facilities. Shortly afterward, the Geological Survey established its new headquarters in Chungking while the National University of Peking was forced to move to Kunming (Yunnan). Having been prevented from joining either of the two institutions, he remained in the old Chinese capital, continuing with his reading and writing. Shortly before the Sino-Japanese conflict he began researches on his "pulsation theory," and during a period of five or six years he completed four large volumes on *Paleozoic formations in the light of the pulsation theory,* which were published by Henri Vetch in Peiping. The last volume appeared in 1938, when fierce battles were being fought both in North and Central China. Following Pearl Harbor in 1941, he was interned by the Japanese, who housed him in the old British Embassy in Peiping. His health began to decline, and his mind became enfeebled. After the Japanese surrender, the authorities of the Geological Survey moved him to their premises at No. 3, Feng-sheng Hutung, where he died at the age of 75.

The Department of Geography, Northwestern University, announces a graduate field course to be given 26 August to 28 September. The field centers on Lannon, Wisconsin, 15 miles northwest of Milwaukee, an area containing examples of virtually every glacial form. The course will be limited to 20 students. For further details address: Secretary, Department of Geography, Northwestern University, Evanston, Illinois.

Karl v. Frisch, professor of zoology at the University of Munich and a native of Austria, has written as follows in a letter dated 14 January 1946, from St. Gilgen (Salzburg), Brunnwinkl, in the Austrian Alps:

> . . . work has never stopped in our laboratory. Only since the destruction of buildings has work become nearly impossible in the Munich Institute. I evacuated before this happened and for several years have carried on my scientific work nearly exclusively here on our old farm. To Munich I go only occasionally, for administrative purposes and discussions with my co-workers. Classes have not yet begun. . . .
>
> I have only partial information on my colleagues. . . . When the bomb attacks became too bad, (Alfred) Kühn transferred the Kaiser Wilhelm Institute for Biology to Hechingen. He has now moved the Institute to Tübingen, where he has also taken on the direction of the Zoology Department of the University. As you know, many departmental headships have become vacant due to dismissal of their incumbents. (Otto) Koehler (formerly Königsberg) was in Denmark when the (Russian) occupation came. I have not heard of him since. Goetsch is in Austria and I hope to see him within a few days. He was last in Breslau. Buddenbrock, who was in Vienna, cannot go back there since he is German. I understand that he is to go to Heidelberg where (Paul) Krüger has been dismissed. It is not yet decided who will take Seidel's position in Berlin. He has been dismissed. Hartmann lived on his farm in the Bavarian Alps. Perhaps he will return to the Kaiser Wilhelm Institute. It is terribly sad that (Fritz) Wettstein died of the flu in Trins without any medical care. Renner is well. A few days ago I received a letter from him from

Jena. (Otto) Mangold too is one of those who were dismissed.

. . . I am much handicapped in my work by the loss of my scientific library, particularly reprints. It would be a great help if you on occasion (when it is possible to mail printed matter) could see to it that I were sent papers on the fields of sensory physiology, psychology, and apiculture.

Last summer I was much occupied again with the "language" of bees. Very unexpected results have turned out. . . .—*Curt Stern* (University of Rochester).

Elections

The American Academy of Arts and Sciences closed its 166th year on 8 May with the election of new fellows and seven honorary members. The scientists among the new members include: James Gilbert Baker, Harvard College Observatory; John Landes Barnes, head of the Department of Applied Mathematics, Tufts College; Subrahmanyan Chandrasekhar, professor of theoretical astrophysics, Yerkes Observatory; Lee Alvin DuBridge, head of the Department of Physics, University of Rochester; Julius Adams Stratton, professor of electrical engineering, Massachusetts Institute of Technology; Paul Doughty Bartlett, associate professor of chemistry, Harvard University; Walter Hugo Stockmayer, assistant professor of chemistry, Massachusetts Institute of Technology; William Loren Batt, president of S.K.F. Industries, Washington, D. C.; Howard Wilson Emmons, associate professor of mechanical engineering, Harvard University; Roland Frank Beers, research associate in geology, Massachusetts Institute of Technology; Cornelius Searle Hurlbut, Jr., associate professor of mineralogy, Harvard University; Ralph Erskine Cleland, head of the Department of Botany, Indiana University; George Wells Beadle, professor of biology, Stanford University; James Lee Peters, curator of birds, Museum of Comparative Zoology, Harvard University; Kenneth David Roeder, assistant professor of zoology, Tufts College; John Franklin Enders, associate professor of bacteriology and immunology, Harvard Medical School; Charles Alderson Janeway, assistant professor of pediatrics, Harvard Medical School; Siegfried Josef Thannhauser, clinical professor of medicine, Tufts College; Stanley Smith Stevens, associate professor of psychology and director of the Psycho-Acoustic Laboratory, Harvard University; Otto Eduard Neugebauer, research professor in the history of science, Brown University; Jan Hendrik Oort, director, Leiden Observatory, Holland; Bertil Lindblad, director, Stockholm Observatory, Sweden; and Rudolf Florin, professor and director Hortus Bergianus, Stockholm.

Howard M. Jones was elected president of the Academy, and Lewis Don Leet, corresponding secretary.

The New Orleans Academy of Sciences held its 93rd annual meeting at Tulane University, New Orleans, 26–27 April. The new officers for 1946–1947 are: J. Nelson Gowanloch, Louisiana Department of Wild Life and Fisheries, president; William T. Penfound, Tulane University, vice-president; Walter G. Moore, Loyola University, secretary; Thomas T. Earle, Newcomb College, treasurer; and Garland F. Taylor, Tulane University, curator.

The retiring president, L. J. Pessin, presided at the general session which was followed by sectional meetings for the presentation of 36 scientific papers.

The Medical Library Association held its 45th annual meeting in New Haven, Connecticut, 25–27 March. The following officers were elected: Walton B. McDaniel, II, College of Physicians, Philadelphia, president; C. Abbott Beling, Newark, vice-president; Heath Babcock, New York State Medical Library, Albany, secretary; and Jurgen G. Raymond, New York Academy of Medicine, New York City, treasurer.

The program featured reports on the Army Medical Library, a symposium on "International Cooperation," and addresses on "Training for Medical Librarianship" and "British Medical Libraries in Wartime."

It was voted to hold the 1947 convention in Cleveland, Ohio, the exact date to be determined later.

The South Carolina Academy of Science held its 19th annual meeting in Charleston, South Carolina, 27 April. The following officers were elected: Velma D. Matthews, Coker College, president; J. E. Copenhaver, Sonoco Products Company, vice-president; and Charles F. Poole, U. S. Vegetable Breeding Laboratory, secretary-treasurer. F. W. Kinard, J. R. Sampey, G. Robert Lunz, Margaret Hess, and F. H. H. Calhoun were elected to the Council.

The meeting, the first since 1941, was attended by more than 100 persons, and 23 papers were presented.

The Crystallographic Society held its spring meeting at Smith College, Northampton, Massachusetts, 21–23 March. The meeting was attended by 50 out of its total of 150 members. The following new officers were elected: M. J. Buerger, Massachusetts Institute of Technology, president; J. D. H. Donnay, The Johns Hopkins University, vice-president; and William Parrish, Philips Laboratories, Inc., Irvington-on-Hudson, New York, secretary-treasurer.

A detailed discussion of the proposed new *Journal of Crystallography* resulted in the adoption of eight tentative resolutions regarding publication, editorship, international representation, etc. These matters will be discussed further by members of the Society, the American Society for X-ray and Electron Diffraction, and British crystallographers and X-ray workers in a conference in London next July, to which scientists from several other countries have been invited.

The secretary was instructed by almost unanimous vote to send the following letter to the President and the Senators on the McMahon Committee:

A majority of the membership of the Crystallographic Society is in favor of legislation which places control of the atomic energy program in civilian hands.

Proposal for an American Institute of Biology

The formation of an organization to serve the interests of biologists appears to be now on the way to realization. The Executive Committees of the Union of American Biological Societies and of the American Biological Society met at intervals during the past year to discuss problems facing biologists and to consider ways and means for securing more thorough cooperation among them. The matter had already been brought up at an informal gathering called by E. G. Butler, then president of the UABS during the Cleveland meetings of the AAAS, two years ago. Detlev Bronk was asked by the group to collect information for presentation at a later appropriate time. This was decided to be at an open meeting called by the UABS and ABS on the occasion of the recent St. Louis meetings of the AAAS and several of the national societies.

Dr. Bronk presented the need of an institute not only for the assistance of biologists professionally and scientifically but also for studying the problem of securing adequate training for those who plan to take up biology as their life work. He also stressed the need of making the profession of modern biology conducive to attracting the best brains of the country. It is highly desirable to have as close cooperation as possible among all operating organizations. A main feature is the importance of closely relating the proposed Institute with the National Research Council and having the Institute serve to strengthen and supplement the activities of the Council. Several services which such an institute can render are outlined in a recent article in the February issue of the *American Naturalist*. Dr. Bronk also outlined an immediate procedure of having the group already engaged draw up definite plans and secure necessary funds.

Following general discussion it was unanimously voted to have the original Cleveland group, with several others, form an initiating committee to proceed with the development of an American Institute of Biology. This committee is to invite other interested biologists who are in a position to contribute the needed time and energy. The membership of the enlarged committee will be selected with a view toward complete geographical distribution and adequate representation of the principal biological societies.

The meeting then adjourned with expressions of enthusiastic approval of the project as proposed.

Subsequent to the St. Louis meeting, several members of the initiating committee have met, and steps are being taken to secure necessary funds and to prepare tentative plans for organizing.

The project has been presented by R. F. Griggs, chairman of the Division of Biology and Agriculture of the National Research Council, at a recent meeting in Washington, to 26 representatives of the constituent national societies who approved.

At the meeting of the Council of the UABS in St. Louis the following statement was approved by the Council, which comprises 39 national societies:

To endorse the formation of an organization of biologists on the basis of the two following general propositions:

1. To provide a means for executing our public responsibilities as biologists and scientists, so that the freedom essential to the progress of science and public welfare will be insured.

2. To safeguard the professional interests of biologists and to assist in providing the material means for the promotion of biological research; also to provide such services as may be necessary to facilitate this program.

The statement was presented to several of the societies convening with the AAAS at St. Louis. It was officially endorsed by the Genetics Society of America, the Botanical Society of America, the American Society of Zoologists, the American Microscopical Society, and others. It was also endorsed by the American Association of Anatomists at their Cleveland meeting a week later. At the meetings of the Federation of Experimental Biology held at Atlantic City in February the proposition of an Institute was presented to the American Physiological Society, which passed a resolution recommending such action. Finally, a letter was received expressing the interest of the Biological Section of the Royal Society of Canada in the proposed American Institute of Biology.

It is expected that plans as they are being developed will be presented in the near future, together with a list of members of the initiating committee.—*Robert Chambers* and *J. S. Nicholas.*

Recent Deaths

Louis Slotin, 35, a native of Winnipeg, Canada, died at Los Alamos 30 May, as a result of an accident 21 May while working with fissionable materials. The nature of the accident was not disclosed by N. E. Bradburg, project director, but he credited Dr. Slotin with dispersing the materials at the time of the accident, so that more workers were not affected. Seven other scientists and technicians were hospitalized but are reported to be recovering. Dr. Slotin, who received his Ph.D. from the University of London, went to Los Alamos from Oak Ridge in December, 1944. He had been at Oak Ridge for about a year.

Letters to the Editor

Rains of Fishes—Myth or Fact?

Being particularly interested in all the unusual things that fishes do and that happen to them, I have for over 40 years been collecting and publishing accounts of such matters, from laymen observers, from scientific men, and from my own experience. In all these articles I have sought to evaluate the evidence and to give credence accordingly. My reports are accredited by my scientific colleagues everywhere.

In the *Atlantic* for April 1946, Bergen Evans, professor of English at Northwestern University, writes that "the little fishes that come in heavy storms are one of the most delightful . . . myths," and further on he refers briefly to one report of a "rain" in my first article (1921). This particular fall occurred in India in 1830. The English reporter had the 10 Indian farmers who saw it attest their reports in 1-2-3 order before a magistrate, and their accounts are so printed in my article. Of Nos. 5 and 8 Prof. Evans says: "Some of Dr. Gudger's more reliable witnesses make the interesting point that the fish that descended on them were headless, rotten and partly eaten—suggesting birds to the incredulous, and God knows what (a rain?) to the credulous."

What Prof. Evans does not note is that these two "more reliable witnesses" also state that some of the specimens were "fresh." Furthermore, he also fails to note that, of the eight other (unreliable?) witnesses, five state that they saw the fishes fall, and No. 5 (a "reliable" witness above) had a fish fall on his head. Furthermore, three men (including Prof. Evans' No. 5 above) picked up fresh fishes and carried them away. All these and the accounts of 43 other reporters in this article (among them James Prinsep) are disregarded by Prof. Evans. He picked out the evidence he wanted, but was "incredulous" of all the other.

In my first article (1921), which seems to be the only one of the four seen by Prof. Evans, I made an attempt to evaluate the evidence. Some accounts were put down as hearsay and some as hearsay pretty well attested. Others come from men who found previously dry receptacles filled with rain water and fishes—among them, James Prinsep (1833), long secretary of the Royal Asiatic Society of Bengal, who "found a small fish, which had apparently been alive when it first fell in the brass funnel of my pluviometer at Benares, which stood on an isolated stone pillar, raised five feet above the ground in my garden." Some of the various reporters saw the fishes fall, and some had the fishes strike their heads or bodies. Among those who did not witness the falls were scientific men of high standing and veracity who, after carefully investigating the alleged rains, accepted the accounts as credible and whose printed word is today accepted by scientific men. Prof. Evans makes no mention of these.

The explanation is to be found in the action of whirlwinds and waterspouts and possibly of strong typhoon and monsoon winds. A "twister" or whirlwind starts in front of an approaching storm, and as it gains in size the "snout" elongates and approaches the water. This, caught by the whirling wind, rises up in a cone. The two unite, and the swirling column moves along, picking up water, fishes, and any other fairly light objects at or near the surface of the water.

I have seen waterspouts off Beaufort, North Carolina, and numerous ones in the Florida Keys west of Key West. In these latter, on a day in July 1914, at the Marquesas Atoll, a huge waterspout was seemingly headed for the yacht on which I had been left as shipkeeper, but when near at hand it fortunately sheered off and passed by about 100 yards away. To this day I have a vivid recollection of the irresistible power of this whirling wind and water. A natural history correspondent in Louisiana (E. A. McIlhenny, of Avery Island) once wrote me of a small waterspout on a fresh-water distributary in the Mississippi delta, which broke just in front of his fishing boats and then filled boats with water and fishes. He knew of other like phenomena in that region. Such a waterspout might pick up dead fish (if such were present) as well as live ones. Everything movable would be sucked up in the whirling vortex. Furthermore, whirlwinds, originating inland, will not only progress over land, picking up various objects, but over ponds and lakes—becoming waterspouts. As such they will there pick up frogs, fresh-water fishes, snails, etc. and carry these away over the land. Sometimes the fishes are found in a long, narrow, fairly straight row over some distance, evidently having been dropped as the waterspout progressed over the country with lessening speed and carrying power.

When the waterspout or whirlwind, with its load of fishes, breaks, or when these and the typhoon and monsoon winds lose their velocity to a point where their carrying power is less than the pull of gravity on the fishes, water and fishes will fall as a "rain of fishes."

In my four collective articles about 78 reports are noted. Their time span is about 2,350 years, and their range in space, the six continents and various islands in the two great oceans. Recorded are rains from Canada (5), United States (17), England (5), Scotland (9), Germany (11), France (1), Greece (1), Paroe Islands (1), Holland (1), India (13), Malaya (2), East Indies (2), Australia (7), South Africa (1), South America (1), Scandinavia (1). These accounts have been collected from works on meteorology, history, travel, and natural history and from various scientific journals, mainly those devoted to natural history in general. These accounts were written by all sorts and conditions of men—ordinary citizens, persons interested in natural history, and scientific men of high reputation for veracity and for accuracy of observation. Among the latter are James

Prinsep, already mentioned (1833); C. W. Grant (1838), of the Bombay Engineers; J. E. Dekay, in his *Fishes of New York* (1861); Pieter Harting (1861); Sir Emerson Tennent, in his *Natural history of Ceylon* (1861); Count Castelnau, the ichthyologist (1861); E. Warren, of the Natal Museum, South Africa (1909); Alexander Meek, of the Dove Marine Laboratory (1918); and J. D. Ogilby (1907) and A. R. McCulloch (1925), well-known Australian ichthyologists. These men not infrequently narrated these accounts before scientific societies and later published in scientific journals.

Most of the nonscientific observers and some of the scientists had no knowledge of what other men in their own lands and especially in foreign countries had seen and written about. Some of the observers had seen the fishes while falling, some had been struck by the fishes, and some had eaten of the freshly fallen fishes. The mass of evidence is as prodigious in volume as it is widespread in time and space. To disregard all this evidence ranging from hearsay to scientifically attested, and to brand as "credulous" all those who, from personal observation or after much study of published accounts, accept much of it as credible, seems, as I wrote in Article I, to indicate a refusal to consider the evidence offered or an inability to evaluate it.

To my very great regret I have never witnessed a rain of fishes, as I have never seen some of the other unusual and extraordinary things about fishes of which I have written in the past 40 years. But if such things have not been physically impossible, and when after careful and critical consideration of the reports (from hearsay to scientific) from widespread sources the world around and from many reputable observers (some known to me personally)—reports which in detail corroborate each other, then I have ample justification for giving them credence, and so I still believe that:

Fishes fall from the sky with rain.

E. W. GUDGER

American Museum of Natural History

Geopathology or Ethnopathology?

In referring to Francis Dieuaide's article (*Science*, 1945, 102, 656), Frederick Sargent, II (*Science*, 1946, **103**, 316) states that "geopathology" is really a branch of biometeorology.

Actually, Dr. Dieuaide's "geopathology" has basically very little to do with biometeorology. True, climate, topography, food, and habit are correlative factors in both; however, they are not the principal factors in "geopathology." Dr. Dieuaide, among others, specifically mentions the "effect of social conditions" and "perhaps hereditary racial traits." In this connection it might also be mentioned that it is somewhat puzzling why Dr. Dieuaide appears to infer that hereditary racial traits are only of secondary importance.

Since we have the opinions of a medical man and a biometeorologist, I wonder if it might not be wise to call upon an anthropologist as an arbiter in this argument. I frankly doubt if anyone would care to lead with his chin. The fact remains, as Dr. Dieuaide very correctly pointed out, that "Geopathology is in its infancy." Nevertheless, it is my personal opinion that Dr. Dieuaide's article is laudable, in spite of the fact that a few minor comments appear debatable.

First, I believe that *Natural resistance and clinical medicine*, by Perla and Marmorston (Little, Brown, 1941) covers a good many of the problems mentioned by Dr. Dieuaide. Second, I believe that American medicine has been somewhat asleep in this regard. It had been my good fortune to obtain several papers in Japan prior to the war which dealt with some studies and research in this field. As a matter of fact, the Imperial Japanese Armed Forces collaborated in some of those studies.

Finally, in answer to Dr. Dieuaide's proposed term, "geopathology," I wonder if the term "ethnopathology" might not be more specific.

HERBERT LIEKER

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A New Pennsylvania Meteorite

Recently one of my students, C. R. Bruce, brought to the laboratory for identification a specimen which had been resting in family cupboards for 61 years. The story was that in September 1886 a man was cutting corn on the Deutihl property, two and a half miles southwest of Bradford Woods, or seven miles northwest of Pittsburgh in Allegheny County, Pennsylvania. He heard an explosion and a rushing noise and ran to the home of George Hillman, who, on going to the field, found the specimen imbedded in the road and still warm. It has been in possession of the family ever since and is now owned by Mrs. Charles Amsler of Baden, Pennsylvania.

Inasmuch as it is in private hands, no opportunity has been afforded for detailed study, but preliminary examination indicates that it is a true stony meteorite or aerolite. As such, it is of considerable interest since it is the first recorded aerolite found in Pennsylvania. R. W. Stone (*Meteorites found in Pennsylvania*. Pennsylvania Topographic and Geologic Survey, Bull. G 2, 1932) lists five meteorites found in the state, but all these were of the metallic type, or siderites.

The Bradford Woods meteorite measures $55 \times 65 \times 85$ mm. and weighs 762 grams. It is shaped somewhat like an old-fashioned pan biscuit with one smooth, curved surface like the biscuit top and three more square faces like the broken faces of a biscuit. The surface has the glazed, varnishlike, pitted surface characteristic of meteorites and is nearly black. It would seem that it is a part of a smooth, pebblelike, elliptic body which, as it reached the earth's atmosphere, exploded, the broken surfaces becoming fused and pitted in the rush through the atmosphere.

A freshly broken corner of the mass made it possible to examine its mineral composition. It is made up of fine-grained, greenish, silicate material which is highly birefringent and has a high index of refraction and an

obscure cleavage. This is probably olivine. Accompanying the silicate is a small amount (possibly 2 or 3 per cent) of metallic iron, which is also visible on the unbroken surfaces. The specific gravity of the whole specimen is approximately 3.4.

From this preliminary examination it is evident that the meteorite is an aerolite with a small amount of metallic iron and may possibly be classed as an olivine achondrite.

It is hoped that the specimen eventually may be acquired by the Carnegie Museum of Pittsburgh or some other public repository so that a detailed petrographic and chemical study of it may be made.

HENRY LEIGHTON

Department of Geology, University of Pittsburgh

Another Superior Pith for Free-hand Sections

Mention of a pith other than elder (*Science*, 1946, 103, 112) prompts the writer to communicate further information in that respect.

Botanists or plant pathologists in tropical or equatorial regions will find an advantageous substitute to elder pith in cassava (*Manihot utilissima* Pohl). It is even the writer's view that the latter is decidedly superior to the former in several respects.

As in the case of *Tetrapanax papyriferum* Koch (above reference), cassava pith has no vascular bundles or hard tissues. Moreover, when used dry it cuts beautifully under the razor, leaving a sheeny surface very soft to the touch. It can be sectioned very thinly without disintegrating, as does that of elder, in like circumstances.

The reason for this can be found in comparing the texture of both piths. Dried cassava pith ready for use has, in cross-section, cells measuring 160–250 μ by 100–150 μ. The cells are larger in the center than outwards and gradually decrease in size in that direction. In longitudinal section the dimensions are contrariwise uniform and vary throughout from 25 to 60 μ. Thus, were it not that they are organized in a tissue, the cells would be lenticular in shape, whereas elder-pith cells are globular and of dimensions somewhat larger than the above.

Extraction of the pith is quite simple and offers no difficulty whatsoever. Cassava stalks should be chosen straight and when the plants are fully mature. They are cut in lengths of about 30–40 cm. A stick of the diameter of the pith is inserted at one end of the fragment. Pushing the stick forces the pith out at the other end in a contorted rod. When straightened out, the rods are left to dry and are then ready for use. The rods can be obtained of a diameter up to 1.5 cm., but they are more usually of 1–1.2 cm., which is quite sufficient for sectioning with a hand microtome.

For cutting small objects the pith can be carved, while in the hand microtome clamp, similarly to paraffin blocks.

The writer has had such satisfactory results with cassava pith that elder pith has been totally discarded.

R. L. STEYAERT

Bambesa, District Uele, Belgian Congo

Book Reviews

The endocrine glands. **Max A. Goldzieher. New York: Rudolf Schick, 1945. Pp. 49. $1.20. 5 wall charts, $6.50 ea.**

Dr. Goldzieher has spent much of a long and productive lifetime as an investigator and practitioner of endocrinology. A strikingly large part of his erudition has come to graphic epitomization in a series of five wall charts that set forth much of our fundamental knowledge of the subject. The charts are expounded in an accompanying brochure that does credit to the author but not the publisher: the illustrations are distressingly muddy.

For the most part the text is accurate and clearly expounded. Whether, however, the rather elaborate diagram setting forth glandular relations is more instructive than confusing might be questioned. The picture representing Addison's disease is misleading in that the characteristic pigmentation is exaggerated. Froelich's disease is ascribed to anterior pituitary disorder rather than to its fundamental neural cause. The illustration of the normal thyroid gland is seemingly a poorly executed section of a colloid goiter. Finally, the curious error is made in the text of expressing the output of adrenal steroids in misused terms of gammas instead of milligrams as is correctly done in the chart itself. These are all errors that can be corrected easily in the revised edition to which the material by its general excellence is abundantly entitled. The charts should be of much aid in the teaching of endocrinology.

R. G. Hoskins

Harvard Medical School, Boston

Endocrine man: a study in the surgery of sex. **L. R. Broster. New York: Grune and Stratton, 1945. Pp. xi + 144. $3.50.**

On the internal evidence of Broster's book the author is a naturalist turned surgeon. He writes both as a biophilosopher and as an authority on the subject of the sexual functions of the adrenal glands. In the latter capacity, the work is satisfying, affording, as it does, an excellent epitome of the contributions of himself and colleagues on a difficult subject. The chief adverse criticism that might be offered in this connection is that oversweeping claims are made for the specificity of the fuchsin-dye method for the identification of the sex-hormone-producing cells of the adrenal cortex.

The exposition of the biology of man is less acceptable. Broster plays fast and loose with the concept of instincts. He overfacilely ascribes the impetus of integrative evolution to three "instincts," those for growth, self-preservation, and reproduction. He then further equates growth with the pituitary gland, self-preservation with the adrenal glands, and reproduction with the gonads. While it is true that the pituitary does furnish a growth hormone, it is also true that growth is promoted by numerous other factors. Likewise, preservation is promoted by many other structures than the adrenal glands. On the whole, the book may be rather well characterized in the writer's own words as "a kaleidoscopic picture of impressions gathered in the pursuit of the practical clinical application" (of endocrinology). The story of evolution is interestingly told but leaves the author's amateur standing essentially unimpaired.

R. G. Hoskins

Harvard Medical School, Boston

Transmission lines, antennas and wave guides. **Ronold W. P. King, Harry Rowe Mimno, and Alexander H. Wing. New York: McGraw-Hill, 1945. Pp. xv + 347. $3.50.**

Developments in the field of antennas and transmission lines underwent a marked change during the years from 1930 to 1940. New concepts and new researchers appeared on the scene, impelled by the development of frequencies far above the old range. What had been a purely theoretical field in electromagnetics suddenly became an integral part of the very practical communication and detection devices developed during the course of the war.

This text is the joint effort of three prominent specialists in the field of ultrahigh frequencies. Alexander H. Wing, in the first chapter, skillfully covers the field of high and ultrahigh-frequency transmission lines. Ronold W. P. King covers the next two chapters on antennas and UHF circuits and provides most of the original material. The brief concluding chapter on wave propagation is by Harry Rowe Mimno.

The authors have contributed materially to the rapid development of the field, and much of the material in this book is original. The body of the text consists of the lecture material from a part of the pre-radar course given to officers in the Armed Forces by the staff of the Cruft Laboratory and is very direct in its approach. Mathematical rigor is preserved, but there are several places where a fuller exposition could be justified, especially in the matter of approximations. In addition, some of the conclusions, such as those to be found in the section on Poynting Vector and Effective Cross Section, are not universally accepted, and little or no mention of the other versions is made except in the references.

It must be concluded that this is an advanced text which must be accepted per se by the uninitiated unless they have time to make a careful study of the references. In fact, it is a most unique example of transference of the skill and learning of the authors in a matter-of-fact way to hundreds of clever young men for the very practical purpose of operating radar and similar devices. The results were excellent, and this volume will serve as a standard of comparison for years to come.

A. E. Vivell

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